

1881." M. Krupp does not give any details of the experiments on which he professes to have founded his tables, or acknowledge any kind of assistance from any other author. He remarks that for a long time the resistance of the air was supposed to vary as  $v^2$ , then to depend upon two powers of  $v$ , and afterwards to vary as  $v^3$  or  $v^4$ . Experiments have shown that these so-called laws of resistance are not good for all velocities. "Cette expérience devait le faire paraître utile de trouver une nouvelle méthode pour le calcul des vitesses restantes" (p. 16). And again, "Un tel tableau pour différences de vitesse de 10 cm. a été établi par l'usine Krupp au commencement de l'année 1880" (p. 18).

M. Krupp's tables are precisely the same as those that have been used in England since 1871, except only that French replace English measures, and that a small reduction of the English coefficients of resistance has been made throughout. Taking one of Krupp's examples (last page) 11/6/79, where  $d = 355$  mm. = 13'977 inches;  $w = 525$  kilos. = 1157'43 lbs.; commencing velocity 490 m.s. = 1607'64 f.s.; remaining velocity 415 m.s. = 1361'57 f.s., at distance 2384 m. = 7821'6 ft.; weight of 1 cubic metre of air = 1'200 kilos, M. Krupp finds from his table 411'8 m.s. for the remaining velocity instead of 415 m.s. given by his experiment. My table gives a remaining velocity of 405'7 m.s. But supposing we reduce the coefficients of resistance in the proportion 99'9 : 109'0 given by the experiments made with the 5-inch gun (solid shot), then we obtain 412'0 m.s. for the required remaining velocity, which is nearly the same as 411'8 m.s. obtained by the use of Krupp's table. Again, taking the experiment 6/8/79 with a projectile 400 mm. in diameter, commencing with a velocity 533'4 m.s., M. Krupp finds a remaining velocity of 447'0 m.s. by the use of his table, while I obtain 440'4 m.s. and 443'8 m.s. is given as the result of experiment. But if I reduce all my coefficients as before in the ratio 99'9 : 109'0, then my table gives 447'4 m.s. as the remaining velocity, which agrees with M. Krupp's calculations. Hence it appears that M. Krupp claims by these tables that his guns of 1880, on the average, give a degree of steadiness about equal to that given by the best of the four English experimental guns used in 1867-68. I have not much confidence in the accuracy of velocities measured at a distance of near one mile and a half from the gun by an instrument not specified, but I have used these data as a means of indicating to what extent the tables give different results. As a test of the tables I should much prefer a careful determination of the commencing velocity of the shot, and the time of flight to some known distant point, where all the times were measured by a single instrument.

I or further information I beg leave to refer M. Krupp to (1) "Tables of Remaining Velocity, Time of Flight and Energy of various Projectiles, &c.," 1871; (2) to the *Proceedings of the Royal Artillery Institution*, Woolwich, September 1871, p. 382, &c.; (3) *Ib.*, April 1872, p. 1, &c.; (4) *Ib.*, December 1877, p. 250, &c.; (5) "Treatise on the Motion of Projectiles, &c.," 1873; (6) "Principles of Gunnery," by Major Sladen, R.A., 1879; (7) "Handbook for Field Service" (R.A.), 1878; (8) "The Construction of Ordnance, &c.," p. 359, &c., 1877; (9) "Reports on Experiments, &c., 84/B/2853," 1879; (10) "Final Report on Experiments, &c., 84/B/2909," 1880; and (11) and (12) "Manual of Gunnery for H.M. Fleet," 1880. And since that date my "General Tables" have been reprinted in four different books.

Since the above was written, I have noticed that the introducer of the Navy Estimates, 1884, remarked:—"The old breech-loader had been found to be of no more use than a muzzle-loader, and the Government had adopted a gun twice as long as the old form of breech-loader." I always understood that the profitable use of the new slow-burning powder required a long barrel, and that the breech-loading arrangement was introduced be-

cause it permitted the use of a longer barrel on shipboard than could be employed with muzzle-loading.

March 22

FRANCIS BASHFORTH

### THIRD NOTE ON THE ELECTRICAL RESISTANCE OF THE HUMAN BODY

IN two previous communications last year, I showed that the amount of this important basis of rational electrotherapeutics had been enormously overstated. Since then I find it given in the new edition of Rosenthal's "Elektricitätslehre," published in the current year, as about 5000 ohms, and, to my surprise, so competent an observer as my friend Prof. Dolbear, in Lockwood's "Handbook of Electric Telegraphy," states it vaguely as from 6000 to 10,000 ohms. On the other hand, Count Du Moncel, in his paper on the conductivity of imperfect conductors in the *Annales de Chimie et de Physique*, vol. x., 1877, approaches more nearly to the real value in stating it from wrist to wrist to vary from 350 to 220 kilometres. This is probably the Swiss unit given in Clark and Sabine's tables as equal to 10'42 ohms or thereabouts. Both Rosenthal and Du Moncel furnish internal evidence that their excessive estimates were due to imperfect contact through the skin: for the former speaks of using fifty chromic acid elements of two volts E.M.F. each; whereas the current from this large battery, with proper contact, would be utterly unbearable to the patient, if not dangerous. The highest current I have seen employed was from twenty-two of these cells through less than 2000 ohms resistance. It was done against my advice, and produced a large carbuncular boil at the nape of the neck, where the negative pole was applied. I have since then completely modified my method of making the skin contacts, and no similar accident has occurred.

Even with a far smaller current, namely, that of eight Daniell cells and small platinum electrodes, of which the size is accurately given, namely,  $4\frac{1}{2}$  by 3 cm. (roughly, the length of two shillings side by side, and the breadth of a florin), Du Moncel produced a similar though much more serious accident. The current was passed at intervals for an hour and a half from wrist to wrist, the patient being a lady, and afterwards for shorter periods in the opposite direction. "On withdrawing the electrodes," says the writer, "to my great astonishment I found, on the parts of the wrists where my electrodes had been applied, very pronounced scars resembling burns produced by an acid or a caustic. These scars, to the number of three at the negative pole, were large and deep. At the positive pole they were very small, and thirty-two in number. During the first two days after the experiment no inflammation supervened, but on the third day it began about the negative scars, and it was necessary to have recourse to poultices, which were kept up for a month; even then the sloughs were not detached." It is satisfactory to find that no permanent harm was done; but it is evident that the excessive resistance recorded, amounting at times to 3500 ohms, was mainly due to this cause. It is perhaps not to be wondered at that the scientific Count should have relinquished this branch of his investigation.

With hands soaked in strong brine, and then enveloped in a thickness of flannel wetted with the same solution, bandaged surgically over this with a spiral strip of lead at least 30 cm. long and 5 cm. broad, no local accident has ever occurred to me, nor has any local pain been mentioned. But with ten bichromate cells in good order the shock felt at making and breaking circuit has often been considerable. Indeed my tall and athletic clinical clerk, Mr. Shackel, who kindly consented to act as a resistance, noticed that, when being tested from foot to hand (in his case a length of 7 feet) with 1027 ohms resistance, the opposite side of the body was jerked at these instants. In all recent experiments I have never exceeded this E.M.F.,

which is at the outside 20 volts, or about 18 volts as the cells run down.

In all cases hitherto named an ordinary battery current has been employed. In a paper read by me before the British Association at Southport, I named a rotating commutator and also one on the plan of a metronome which I had tried for the purpose of diminishing currents of polarisation by regular inversion. I preferred, however, the rapid manipulation of an ordinary commutating key with the fingers of the left hand until the "throw" of a damped galvanometer was all but extinguished.

At the Southport meeting, however, my friend, Dr. Oliver Lodge, suggested the use of alternating currents of induction, and a telephone in place of the galvanometer, and Prof. Lankester, the President of the Section in which my paper was read, kindly suggested that I should apply to the Royal Society for a grant in aid to purchase the expensive apparatus required for these experiments. The latter suggestion I at once acted on, and met with unconditional refusal on a printed form. Being thus thrown on my own small means, I proceeded to act on the former suggestion, and ordered an induction apparatus of an excellent London maker. But the British workman, if sure, is decidedly slow, and the instrument, though stated to be in a condition of forwardness, is not yet ready. In the meanwhile, in the pages of the *Electrical Review* for January 12, a diagram, description, and wood-cut of a pretty little instrument designed by Prof. Kohlrausch of Wurzburg for the measurement of fluid resistances appeared; by his kindness I was put in communication with the firm of Hartmann and Co. of that town, the makers. They at once forwarded me the instrument, which proves to be beautifully made, and extremely moderate in price. This acknowledgment I owe to the Professor's courtesy towards a stranger, and their briskness in carrying out his wishes. Upon its details it is needless now to insist, it being practically a small induction-coil united to a metre-bridge of platinum-silver wire, with resistances of 1, 10, 100, and 1000 ohms, to be intercalated in the divided circuit. It emits a steady buzz of about 120 vibrations per second, which is reproduced in the telephone by methods well understood. In my first experiments I found the original and the phantom buzz difficult to separate. The former is easily lessened by mounting the apparatus on vulcanised rubber tubing and a solid support. The R. is read off the scale by inspection: towards the left hand or middle of the wire with great accuracy; towards the right-hand end the ohms get squeezed together. When I drew the plug of the 1000 R. my willing student-patient gave a jump out of his two brine baths and said he could not stand it. It was therefore necessary to use the 100 ohm plug. Even with this, however, the results were very remarkable. In this early period of my experiments two illustrative cases may be given. A female patient suffering from diabetes, but otherwise in good health, and able to walk about the ward, gave from foot to foot with an E.M.F. of 3.6 volts, a resistance of 1210 ohms; from right hand to right foot 1350 ohms; and from left hand to left foot exactly the same figure. With the induction current she gave from foot to foot only 473 ohms; from hand to foot 735 ohms on the right, and 750 ohms on the left, side. The difference was so great that at first I suspected instrumental error, but subsequent testings show that such is not the case. The discrepancy of 15 ohms between the two sides was clearly owing to my unfamiliarity with the telephone in place of galvanometer, and has materially lessened with greater experience.

A male patient suffering from dysentery, now perfectly well, gave from right hand to foot with a current of 3.6 volts a R. of 1580, with 6.2 volts a mean of 1510, with 18 volts a R. of 1366. Each observation was taken twice; the first and last agreeing exactly, the intermediate

only differing from 1520 to 1500. This is impossible at times to prevent from the unintentional motions of the patient slightly shifting the level of the brine baths. With the same baths and poles the induction current gave only 590 ohms resistance.

In neither of these cases was there any morbid condition of the muscles tested. The distance was in each case from the external malleolus of the foot to the head of the ulna in the corresponding hand. In recording these results, I prefer, as on the former occasion, to give them at once in their rough state before waiting for a plausible explanation, or endeavouring to procure a fallacious agreement between the two methods. It is clearly not, as a writer in the *Electrical Journal* thought, a case of mere "cable-testing." What I stated then I now reaffirm, that there is some important difference of a physiological character between the human body as a conductor and ordinary fluid electrolytes.

No doubt, as Dr. Lodge suggests, "an alternating current ought to show too low a resistance, because of electro-chemical capacity, which it would treat like conductivity." But the difference is far too great for such an explanation, nor does it occur to this extent in saline solutions. I am at present engaged in testing its amount in physiological fluids, such as blood-serum, ascitic and ovarian effusion, and the like.

A beautiful metre-bridge on Prof. Kohlrausch's pattern, with platinum-silver wire of 3 m. long, has just reached me from Hartmann; with this I am using a "sledge" inductorium of Du Bois Reymond's with three different secondary coils of different lengths and fineness of wire. For the determination of the alternating currents passing I am using the small dynamometer with aluminium wire suspended coil which was shown before the Physical Society, and briefly described in NATURE.

This I shall check by a fine instrument now on its way from Wurzburg, with a single wire suspension and torsion head instead of the more sluggish bifilar method. Ultimately it may be necessary to use a quadrant electrometer.

Even at this stage it is obvious that the fact of the human body being about twice as permeable to induction as it is to low tension continuous currents is of great physiological and therapeutical importance.

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#### INTERNATIONAL WEIGHTS AND MEASURES<sup>1</sup>

ALTHOUGH to some it might appear that the work of the Bureau at Sèvres is perhaps proceeding slowly, yet by reference to the two publications which have been issued under the authority of the Comité International it may be seen that the Bureau is doing its work thoroughly. The extent of the questions investigated is well shown in the first publication issued in 1881 (tome i.), which included papers by the director, Dr. Broch, on the force of gravity, the tension of vapour, the boiling point of water, and the weight of a *litre* of air; as well as independent investigations by Dr. Benoit on Fizeau's dilatometer; by Dr. Pernet, on thermometers; and by M. Marek, on weighing apparatus, &c.

The present publication (tome ii. 1883), to which we would now invite attention, contains accounts by Dr. Benoit of his expansion experiments; by M. Marek, on the methods and results of the weighings made at the Bureau from 1879 to 1881; and by Dr. Broch, on the expansion of mercury. In the experiments on the dilatation of standard measures of length, there has been followed a method attributed to General Wrede. It consists in the first instance in adjusting under two microscope-microscopes a platinum-iridium bar, on which the

<sup>1</sup> "Bureau International des Poids et Mesures." *Travaux et Mémoires*, tome ii., 400 pp. Paris, 1883.